question at present before us. In the result Mr. Gulick completely agreed with me, that it cannot signify *how* or *when* the physiological variation of initial cross-infertility arises; for to whatever *causes* it may be due, and at whatever *time* in the process of varietal divergence it may first occur, it must alike furnish as highly important a condition to the origination of species as Mr. Wallace has eventually himself assigned to it.

I say "eventually," because Mr. Wallace has never before expressed himself to the effect that, in his opinion, cross-infertility is a factor of such prime importance in the origination of species. Why has he never done so? Surely the matter is one of sufficient magnitude to have justified some mention in one or other of the many valuable "contributions" which he has made to the theory of evolution. Or, not to go further than his past criticisms of my own paper on the subject, what pages of controversy he might have saved in this journal and elsewhere by stating, at any time within the last four years, that he had no disagreement with me touching the probable occurrence, and the important consequence, of some degree of infertility characterizing varieties which afterwards, and on this account, develop into species; but merely doubted whether any degree of infertility could ever arise before differentiation of some other kind had begun to take place. Such criticism would have been mild indeed. But hitherto the crown and front of opposition to the theory of physiological selection has been that, in representing cross-infertility as a factor of any great importance in the origination of species, the theory is not only untrue in itself, but tends to "shrivel up natural selection to very small dimensions." Now, however, criticism "changes front." It is no longer denied, but actually upheld, that "selective fertility" is as highly important a "co-operative cause in the origination of species" as I have ever claimed; and the new attack is directed only to a very subordinate point—a point, moreover, which both Mr. Gulick and myself had expressly anticipated, fully discussed, and shown not to belong to "the essence of the theory."[1]

Oxford, December 22, 1890. GEORGE J. ROMANES.

Molecular Dispersion.

IN the notes that appeared in your journal of December 11 (p. 133), you gave a very full account of some papers lately published in the *Bulletin de la Société Chimique de Paris*, on optical dispersion, by Messrs. Barbier and Roux.

This investigation is a remarkable instance of how laborious and intelligent work may be almost wholly thrown away for want of the knowledge of what has been previously done in the same direction. The authors commence their first paper with the astounding statement: "Dispersion has never been studied from the point of view of the relations which connect this physical property of bodies with their composition, their molecular weight, and their chemical constitution." This, however, was attempted by Sir John Herschel more than half a century ago, though with little success: and most of the scientific men who have best elucidated the subject of molecular refraction, such as Mascart, Brühl, and Nasini, have paid some attention also to dispersion. Mr. Dale and I gave numerical values for dispersion equivalents, analogous to Landolt's "refraction equivalents," for CH_2, Cl, &c., as far back as 1866. The dispersion of isomeric bodies was treated by me in 1881; and within the last few years I have communicated papers on the subject of molecular dispersion to learned Societies in England, France, and Switzerland. Some of the substances worked on by Messrs. Barbier and Roux have not, I think, been optically examined before; but the value of their careful observations is much diminished by their having measured two lines of the tin spectrum, instead of the A and H of the solar spectrum, or the α and γ of that of hydrogen; thus their results cannot easily be compared with the hundreds of measurements of dispersion already published by others. They have unfortunately employed Cauchy's formula, and have taken as the "specific dispersive power," $\frac{B}{d}$; that is, Cauchy's B divided by the density of the substance, instead of $\frac{n_a - n_b}{d}$, the difference between the refraction of two lines divided by the density. Brühl commenced to work in the same way, but shows in a paper in *Liebig's Annalen* for August 1886 that the method is unsatisfactory. The generalizations of our present authors are definite, and apparently correct, but they could have been mostly foreseen and explained if they had more clearly grasped the idea of molecular dispersion.

In the early summer I sent Messrs. Barbier and Roux copies of my papers on the subject; on August 1 they acknowledged receipt, and promised to refer in a forthcoming paper to previous work. As the paper reviewed in NATURE appeared in a preliminary abstract in the *Comptes rendus* of July, it is evident that they have yet something to bring before the public.

J. H. GLADSTONE.

17 Pembridge Square, December 27, 1890.

Weighing with a Ternary Series of Weights.

IN a former communication (NATURE, November 13, p. 30) I omitted, in order to avoid prolixity, to show how readily any given number may be expressed in the notation therein proposed. To effect this, it is only necessary to express the given number in the ternary scale, and then, beginning on the right, to substitute for 2, wherever it occurs, − 1, whilst increasing the previous figure by 1. When 3 occurs in the application of this rule, it must of course be replaced by 0, the previous figure being again increased by 1.

Examples:—

41 is in ternary notation 1112, in new notation **11111.**
500 ,, ,, ,, 200112, ,, ,, **1101111.**
71 ,, ,, ,, 2122, ,, ,, **10101.**

Or (still more briefly) in dividing by 3, to express the number in the ternary scale, we may substitute for the remainder 2, wherever it occurs, − 1, and increase the quotient by 1.

Examples:—

425		474		500	
142 remainder	− 1	158 remainder	0	167 remainder	− 1
47 ,,	1	53 ,,	− 1	56 ,,	− 1
16 ,,	− 1	18 ,,	− 1	19 ,,	− 1
5 ,,	1	6 ,,	0	6 ,,	1
2 ,,	− 1	2 ,,	0	2 ,,	0
1 ,,	− 1	1 ,,	− 1	1 ,,	− 1
0 ,,	1	0 ,,	1	0 ,,	1
3 + 27 + 729 − (1 + 9 + 81 + 243)		729 − (3 + 9 + 243)		27 + 729 − (1 + 3 + 9 + 243)	

Bradford, December 13, 1890. J. WILLIS.

PROF. EVERETT's rule (NATURE, December 4, p. 104) is needlessly complicated. All that is necessary is to express the given weight in the ternary scale with digits that are either 0, + 1, or − 1. His example is thus solved:—

3)500
3)167 − 1
3)56 − 1
3)19 − 1
3)6 + 1
3)2 + 0
1 − 1 or 500 = $1\bar{1}01\bar{1}\bar{1}\bar{1}$ ternary
= 729 − 243 + 27 − 9 − 3 − 1.

We thus see generally that, to express weights in units and powers of n, we must be provided with $\frac{1}{2}n$ or $\frac{1}{2}(n - 1)$ balance-

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[1] P.S.—Mr. Wallace alludes to my "standards of scientific reasoning and literary consistency." As regards the former, I am satisfied with a full and independent corroboration by a consistent and a logical mind. As regards the latter, it is enough to quote the concluding words of my reply to Mr Wallace's first criticism of four years ago:—"The *main* feature of the theory is what my paper states it to be—viz. that sterility with parent forms is one of the *conditions*, and not *always* one of the *results*, of specific differentiation. But, if so, is it not evident that all causes which induce sterility are comprised by the theory, whether these causes happen to affect a few individuals sporadically, a number of individuals simultaneously, or even the majority of an entire species?" (*Nineteenth Century*, January 1887). And is it not equally evident, as elsewhere stated, that it does not signify whether the sterility arises before or after the "differentiation" has begun; seeing that, in either case, without the sterility the differentiation (as Mr Wallace now says) will usually fail to proceed to the formation of distinct species? I have no space to discuss Mr. Darwin's views on this subject; but assuredly they are far from those which are expressed either here or in "Darwinism."

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weights of each sort, according as n is even or odd; and that then any given weight can be expressed in a single way only if n is odd.

R. E. B.

I AM glad to have elicited a simple and direct rule for the required distribution, to obviate the necessity of a tedious tentative process, or of the reference to tables suggested in Mr. Willis's first letter. My own rule effected the desired object, but the device of admitting negative as well as positive remainders in the successive divisions by 3 is a decided improvement. R. E. B.'s method is identical with Mr. Willis's second method, and is undoubtedly the best. Its relation to my method is seen by noting that $\frac{1}{2}(2^7 - 1)$ is ternary 1111111, which, being added to $1\bar{1}01\bar{1}\bar{1}\bar{1}$, converts it into 2012000. My rule might have been generalized by adding (instead of *the least* value) *any* value of $\frac{1}{2}(2^n - 1)$ that exceeds the given weight.

In connection with Mr. Willis's suggestion (in his first letter) of tables for finding what number a person has thought of, I may mention that I published through Simpkin and Marshall, nearly forty years ago, a set of 4 cards for this purpose under the name of "Sibylline Leaves," of which a few specimens are still in my possession. Taking advantage of the fact that weights of 1, 3, 9, 27 will make up any integer from − 40 to + 40, that is 81 different integers when 0 is included, the numbers on the cards ran from 1 to 81. The computation consisted in taking 41 to start with, and adding or subtracting 1, 3, 9, or 27, two kinds of type being employed to distinguish between addition and subtraction.

J. D. EVERETT.

The Composition of Sea-Water.

COULD any reader of NATURE who may have given attention to the subject offer some explanation of the fact that the water of the sea contains such a very large excess of sodium salts relatively to salts of potassium?

Many of us have thought about the question, and heard it discussed, but I am not aware that any satisfactory conclusion has been arrived at.

I think it is usually assumed that the salts in the ocean have been mainly derived from the solutions carried in by rivers; solutions formed during the waste of rocks at the surface, or brought up by springs. Alkaline salts in such solutions will be principally due to the decay of felspars; and if we consider the rocks of the earth's crust, we find that the potash-felspars very much exceed in quantity the soda-felspars. It used to be considered that in earlier geological periods the "acid," mainly potash-bearing rocks, so enormously exceeded the "basic" rocks, in which the soda-felspars occur principally, that these latter were relatively quite insignificant in amount. Later petrographical work tends to show that this preponderance may not have been so large as was once supposed, but still there is no question that the excess of the potash-bearing rocks was, and is, very great. We might therefore look for more potash than soda in the drainage waters. We find, however, that in the sea, and in the rivers, the reverse is the case. Instances are, indeed, quoted (Roth, "Chemische Geologie") of rivers with more potash than soda in solution, but only as exceptions, and at points where only granite and gneiss had been drained.

This excess of soda in river-waters may be explained by the fact that though more potash-rocks are exposed than soda-rocks, yet the more rapid decay of the soda-lime felspars causes the proportions of the dissolved salts to be such as we find them; though this would hardly suffice to explain the great difference we find in the ocean.

Some people, again, assume that the composition of sea-water, though it may have been modified by the river-waters, is due to the constituents which were contained in the original first ocean as it condensed on the surface of the cooling earth—the "Urmeer" of the Germans; and Roth points out, in discussing analyses of river-waters and sea-water, that the composition of the former is such that they never could be the cause of the present composition of the latter.

This going back to the original ocean may be, as indeed it seems to be, the only explanation open to us, but it is not without its weak points. If potash-salts so greatly exceed soda-salts in the earth's crust now, we may assume that the same relative proportions existed, more or less, when the whole mass was still gaseous; and it is not easy to see any reason why, when cooling and condensation had allowed of the formation of a mass of molten silicates, the sodium-salts should have remained in great excess in the still uncondensed heated atmosphere out of which the "Urmeer" would eventually be deposited on the cooling crust. Mere difference of volatility of the respective salts would not suffice to account for this; and if we are to take the hypothesis at all, we must perhaps assume that when a low enough temperature had been reached to allow of the combination of the respective elements to molten silicates, the potassium, as the stronger base, would be taken more largely into these combinations by preference to the sodium, which would partly remain in the still intensely heated atmosphere, to be condensed with other vapours at a later period.

M.

Birds' Nests.

IN addition to your "curious places for birds' nests," I give you my own experience between 1842 and 1882 at Highfield House, Nottinghamshire.

Redbreast for four consecutive years on a shelf.
Redbreast in a fern (*Platycerium alcicome*) for four consecutive years.
Redbreast in a *Strelitzia regina* plant.
Hedge Warbler in a tall Fuchsia in a greenhouse.
Chiff Chaff in a fern (two years).
Pied Wagtail on a shelf in vinery (two years).
Flycatcher on hinge of door (ten consecutive years).
Flycatcher on ledge of thermometer stand (three years).
Wren in a Daniels's hygrometer stand.

County Club, Chepstow. E. J. LOWE.

Butterflies Bathing.

IN answer to the inquiry of Mr. G. A. Freeman (NATURE, vol. xlii. p. 545) as to the food and habits of *Papilio macleayanus*, the butterfly which has been observed to visit water apparently for the purpose of performing its ablutions, I may inform him that the species is commonly found about Sydney, where it feeds in its larval condition on the camphor laurel (*Laurus camphora*), and the tender shoots and leaves of the orange. It certainly is not aquatic during any part of its life, nor do the plants upon which it feeds grow near water; the insect simply follows the example of its brothers, depositing its eggs singly, and undergoing the transformations on the food-plant as any reasonable butterfly should. Mr. G. Lyell's note as to the bathing habits of *P. macleayanus* is most interesting, and as far as I am aware the observation is entirely new, although many butterflies of the family Lycænidæ frequent pools on very hot days, settling on the mud at their margins, probably in search of a little moisture. Only recently at Toowoomba, in Queensland, I noticed a number of *Holochila absimilis* settled about puddles formed on the roads by a passing shower.

A. SIDNEY OLLIFF.

Department of Agriculture, Macquarie Street, Sydney,
November 11, 1890.

THE RESEARCHES OF DR. R. KŒNIG ON THE PHYSICAL BASIS OF MUSICAL SOUNDS.[1]

I.

NOT often does it fall to the lot of a scientific man to become the mouthpiece of another whose researches have lasted over a quarter of a century; yet this is the enviable position in which I find myself on this occasion as the spokesman of Dr. Rudolph Kœnig, who is known not only as the constructor of the finest acoustical instruments in the world, but as an investigator of great originality and distinction, and author of numerous memoirs on acoustics. Dr. Kœnig, who has of late made very important contributions to our knowledge of the physical basis of music, using apparatus immeasurably superior to any hitherto employed in experimental investigations of this subject, has on various occasions, when I have visited him in Paris, shown me these instruments, and repeated to me the results of his researches. Important as these

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[1] By Prof. Silvanus P. Thompson. (Communicated by the author, having been read to the Physical Society of London, May 16, 1890.)

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